

and I wish to make out a perfect vindication, hoping never again to be obliged to recur to it. If you will refer to my reply to Peters, you will see that I speak of our difference in R.A. and stop, coming to a full pause. I then take up the subject of Dec., and when through with that, make another period. Then I say, "Thus the matter rested until NATURE pointed out the error, &c." Is not your language about as unlike this as can well be? In response to your wish to be able to tell your readers "how this sudden illumination caused the scales to fall from my eyes," I hope the above explanation will prove full and clear to all.

Your second charge, "hesitancy about the matter," is a new one, and so at variance with truth that necessity, even at the expense of being prolix, compels me to refute it, and to show to the world that this charge is as baseless as the other. How long did I hesitate? I answer, from the time of the eclipse until just two minutes after my arrival at home, when, though very weary and ill, and before I was seated, I consulted "Webb's Celestial Objects" to see how far Alcor was from Mizar. Then and only then was I able to fix on a definite distance between θ Cancri and, as I then supposed, the planet Vulcan, viz., about $7'$. I left Denver the next morning after the eclipse, coming homeward, both by night and day, as fast as steam could bring me, arriving at home on the P.M. of Saturday, before most of the astronomers had left Denver. I immediately despatched a messenger to the Editor of the *Rochester Sunday Morning Herald*, notifying him of my arrival. I was at once interviewed by him, and a full account was laid before his readers by daylight the next morning. Sunday P.M. I was interviewed by a reporter of the *Rochester Democrat and Chronicle*, which paper, the next morning, contained a long account of my observations, a considerable part of which was published in NATURE. As soon as possible I wrote the facts to the Astronomer-Royal, to the *Observatory*, to Admiral Mouchez, and made out my report to Prof. Colbert, of Chicago (the chief of the party to which I belonged), which, with those of the other members, was published in pamphlet form, also a more extended one to Admiral Rodgers, not yet published. Very little hesitancy in this I think.

I left Denver with Professors Colbert and Hough. On the way Prof. Hough asked me several questions regarding the distance between the two stars. I told him I was unable to give their distance in arc, neither could I think of two stars whose apparent distance was the same. I also said to him that the nearest approach to a resemblance which I could then recall were α^1 and α^2 Capricorni, but, not having observed them with such an object in view, would not say that they were sensibly the same. After they had left me—changing to another road—and before my arrival at Kansas City, and before night of the day of starting, the thought came suddenly to my mind that their distance apart was about equal to a little more than half that between Mizar and Alcor, whatever that might be, which could not be ascertained until my arrival at home.

Since the eclipse I have made many observations of θ Cancri and regions adjacent, to see if my judgment would allow me to modify in any particular my observations as made and published. I have even gone to a part of this city where the streets run parallel with and at right angles to the meridian, as they did at our camp, in Denver, and then wait until an imaginary sun some $30'$ west of δ Cancri had the same altitude and azimuth as had the real sun during totality. And, while I am not inclined to make any changes whatever, I will say that it cannot be denied that, as regards the distance and direction from the sun, they can only be considered as rough guesses, though this does not militate in the least against the existence of the new objects. That they are new I know, for they are not there now. I have never made a more valid observation, nor one more free from doubt regarding the genuineness of the objects seen, which, in my opinion, were circumsolar bodies, unquestionably intra-Mercurial planets. The view of them was as beautiful as it was unexpected, and it was with great reluctance that I could break away from the captivating scene. It must be borne in mind that my telescope was filled with a flood of light, with not an object for reference visible, and therefore, when I ran upon these two round red disks, equally bright, and so near together, it is not surprising that they made an impression upon my mind that never will be effaced.

The great field for future astronomical discovery will, without doubt, be the sun and his immediate surroundings. Let no man's prejudice deter him from taking part in such prospective discoveries, for the field promises rich rewards.

Though I have said above that I am not inclined to modify my published estimations, yet I am willing to say as follows:—If I were compelled to change the brightness of the two stars one magnitude, and say whether they were of the fourth or sixth, I should answer, the former. If I were compelled to change their distance from the sun half a degree, and say whether they were $2\frac{1}{2}''$ or $3\frac{1}{2}''$, I should say the latter. Again, if I were compelled to change their direction from the sun, and say a little farther south or north, I should unhesitatingly say the latter, or, as I said in my report to the Naval Observatory, south of west, instead of south-west. And, finally, were I obliged to change their distance apart, and declare whether they were $6'$ or $8'$, I should, without a moment's hesitation, say the former, or about the distance between α^1 and α^2 Capricorni.

LEWIS SWIFT

Rochester, N.Y., December 10, 1879

The Transverse Propagation of Light

IN NATURE, vol. xxi, p. 256, appeared a paper by Mr. Tolver Preston, on which I wish to make a few remarks.

The author does not make himself very clear as to what he supposes the effect of the vibrating molecules of gross matter on the ether atoms to be. From what I can gather, the effect on a small plane receiving the light from an illuminated "point" would be of the following nature:—When the molecule of gross matter was not vibrating, there would be a more or less shaded spot on the plane, but if the molecule vibrated, then this shaded spot would also vibrate in the same time, which would be possible, since during one vibration of the molecule an extremely large number of ether atoms would impinge on it, and therefore, a large number at each portion of its vibration. In what follows I shall suppose that this is the manner in which the light is supposed to be propagated.

1. The atoms are very small; the free paths are very long. In order that the acceleration of the sun on all the planets must be inversely proportional to the squares of their distances, this mean path must be comparable with the radius of Neptune's orbit; and in order that the light of the stars may be visible, it must be comparable with the distance of the furthest visible star. Again, since, as Mr. Preston says, the automatic adjustment to equality of direction is "of such a rigid character, that if the atoms were imagined to be disturbed or made to move in the most chaotic manner, they would, when left to themselves, instantly correct the irregularity," it follows that the time of describing the mean free path must be very much smaller than the "instantly" small time in which they "correct the irregularity." Their velocity, therefore, must be enormous. They must move to the farthest visible star in a very small fraction of a second. That they have a very large velocity also follows from the smallness of the atoms and the magnitude of gravitation. Now the velocity of light on Mr. Preston's theory must be the velocity with which the atoms move, a velocity which, as has been shown, must be enormously greater than 200,000 miles a second.

2. The above supposes the velocity of all atoms the same, which would not be true. If they varied in the same way as in a gas composed of atoms which do not influence one another, then at a distance from the illuminated point, after a few vibrations of the gross molecule, the shaded spot would not vibrate, but would become an elongated shaded spot without motion, and there would be no light at all.

3. The data of the theory are definite, and it therefore ought to be capable of explaining the laws of refraction and reflection, let alone those of diffraction. This it is incapable of doing; for the light that gets through must be carried by atoms which pass through without striking any of the molecules of gross matter; they must therefore pass through without change of direction or velocity, and therefore cannot be deflected.

These are three reasons, each of which by itself condemns the ingenious explanation offered by Mr. Preston.

W. M. HICKS

St. John's College, Cambridge, January 16

Mountain Ranges

It is to be regretted that Mr. Trelawney W. Saunders should make confusion worse confounded by noticing imaginary discrepancies based upon a mistaken assumption of a natural agreement. In his paper "On the Mountains of the Northern and Western Frontier of India," published in NATURE, vol. xxi, p.

96, he takes geologists to task for not making their descriptions to fit in with his delineation of purely superficial features. He reproaches the authors of the "Manual of the Geology of India" with adopting an "antiquated theory" which had been disposed of by his demonstration of a second line of peaks in the Himalayan range. The omission to account for such apparent neglect of recent discovery was solely due to the perceptions of its almost irrelevancy to the matter in hand. The old familiar feature for which Mr. Saunders claims such geographical importance (which the writers were not concerned to dispute) happens to be of quite incidental significance in the mountain-structure, and much more in accordance with "the antiquated theory" than with the independent position Mr. Saunders would assign to it. Also, the fact that the great gneissic axis of the Himalayan range divides into several minor axes west of the Sutlej, and that these disappear under fossiliferous formations before reaching the Indus, will probably be held by geologists as sufficient reason for considering this ground as the natural termination of the range. On the other hand, the fact that there should be a continuous watershed between these terminal ridges and the contiguous ridges of a confluent system of disturbance, will be admitted by geologists as sufficient for a combined hydrographical delineation of the two systems, as proposed by Mr. Saunders. The points of view of the pure geographer and of the geologist are at present so wide apart that it is irrational to represent them as conflicting.

H. B. MEDLICOTT

Calcutta, December 31, 1879

Ice Filaments

THE phenomenon alluded to by the Duke of Argyll in NATURE, vol. xxi. p. 274, is not at all of unfrequent occurrence. I remember having been struck by the beauty of these ice-filaments on dead branches in Epping Forest many years ago, and some friends of mine observed some beautiful specimens of such branches in Surrey some few weeks since. The explanation which I have been inclined to give is the following:—During the moist weather preceding the frosts, the dead branches on the ground become sodden with water; the interstices between the cells of the dead ligneous fibre get saturated by capillarity, and the branches become water-logged. Now if a certain amount of dry weather intervenes between the moist period and the frost, this absorbed water would have time to partially evaporate and leave the branches more or less dry. But if the frost immediately follows the moist period—as pointed out by the Duke of Argyll—there is no time for the drying of the branches, and the interstitial water becomes frozen *in situ*. Under these circumstances the expansive force of the ice would cause it to flow out from every available pore by virtue of its viscosity, and such I take it is the origin of the filaments observed. Those portions of the branches which are protected by bark are sheathed by the latter in such a manner that the ice is prevented from oozing outwards; but my friends who have recently observed the phenomenon inform me that where the bark was partly separated from the wood beneath it so as to leave a small intermediate space, this space was likewise filled with filamentous ice.

All physicists are familiar with the experiment of smitting fragments of ice to great pressure in a steel mould with an opening in it. The ice becomes consolidated by regelation, and flows out of the opening in a continuous thread. The state of affairs in frozen water-logged branches could thus be imitated by having a steel mould sufficiently strong to bear the pressure, completely filled with water and perforated by capillary holes, and then freezing the contents. The ice would, under these circumstances, flow out of the capillary holes in the filamentous form observed, and if a metal band were firmly fastened round the mould so as to sheath a certain zone of the capillary holes, no ice could appear in this zone, which would thus represent the portions of the branches protected by bark.

From the point of view of this explanation, which I venture to submit for the judgment of physicists, the Duke is hardly correct in speaking of this filamentous form of ice as an "ice-crystal."

R. MELDOLA

21, John Street, Bedford Row, W.C., January 23

THE filamentous form of ice-crystal, described by the Duke of Argyll as occurring upon rotten wood when a frost sets in suddenly after moisture, is by no means uncommon also upon chalk and other porous kinds of stone. It appears to arise from

the water with which the body is soaked being extruded by the expansion due to cold when near its freezing-point, and becoming solidified as it passes the surface of the substance. It is, as it were, spun out of the pores of the rotten wood or porous stone. This explanation accounts for the fact, noticed by his Grace, that this form of crystal is not found upon those parts of a decayed branch upon which the bark is unbroken.

Harlton, Cambridge, January 23

O. FISHER

WHILE residing upon the South Downs I observed, during hard frosts, that prisms of ice exuded from small pieces of chalk, and having their sections identical with the piece of chalk. It is clear that the prism was formed by the moisture passing through the chalk by capillary attraction. May not this explain the formation of the filaments described by the Duke of Argyll?

H. KING

The Kangaroo

I NOTICE in NATURE, vol. xx. p. 511, in a lecture on "Tails," the following remarks in reference to kangaroos:—

"These creatures make use of their tails not only sometimes to carry grass, and to a certain extent in their jumps," &c. Permit me to state that the former statement is perfectly erroneous and the latter one is correct only in a very modified degree. Kangaroos *cannot* use their tails to carry grass, and never attempt it, and the use of their tails in jumping is confined to balancing the body, and whatever leverage may be exerted in the swaying of it when in motion. The tail *never* touches the ground in going. Twenty years' observation in three colonies is my authority for saying so.

ALFRED MORRIS

Sydney, N.S.W., December 30, 1879

Chinese Geese

IT may interest some who read Mr. Darwin's note on this variety, to know that there are—or were only a few months ago—a rather large number of hybrids, of apparently all grades, at the Bristol Zoological Gardens. When I was there in September there was quite a respectable flock, pure Chinese being among them.

I have not unfrequently found both the pure variety and hybrids in the country, and have usually found that the people regarded them merely as a variety. The differences mentioned by Mr. Darwin seem scarcely so great as those presented by the Polish fowl—which also, by the way, seems almost to have been regarded as a species by some naturalists of good repute.

LEWIS WRIGHT

The Molecular Velocity of Gases

IN NATURE, vol. xxi. p. 201, which reached me only recently, I find a letter of your correspondent "K," to whom I am much obliged for having pointed out to me an error into which I had fallen, in common with many others. I may quote, *e.g.*, the exhaustive work of Rühlmann,¹ where, in the chapter on the history of the molecular theory, Joule is only alluded to, and immediately afterwards the theory of Krönig is given *in extenso*, without any hint that it is practically identical with that given by Joule in 1848. Having read "R's" letter, I immediately procured the original article of Joule, and I am now ready to admit that Joule's article contains all that is essential to Krönig's method of computing the velocity of gas molecules. It is true, the formula itself as an algebraical expression is not found there, but the calculations given are to all purposes equivalent to the formula.

It is scarcely necessary to add that this makes no difference at all in reference to the contents of my letter in NATURE, vol. xxi. p. 176, referring, as it does, only to the historical footnote.

L. HAJNÍŠ

Prague, Spálená ulice, 2 nové, January 20

Suicide of the Scorpion

SINCE writing mine of the 12th inst. I have, I believe, discovered in Byron's "Giaour" the scientific (?) flight of fancy upon which Dr. R. F. Hutchinson based his *central glowing*

¹ "Handbuch der mechanischen Wärmetheorie."